

Opinion.

Parallelism and Phylogenetic Trees

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Nearly all of Brock's (1988) statements in his criticism of my (Scott 1986) phylogeny of the advanced Ditrysia and Macrolepidoptera merely (but correctly) demonstrate that many character states of one Ditrysia group also independently occur in one or more other Ditrysia groups. But every worker on Ditrysia knows this; I knew of most of these independent occurrences when I wrote Scott (1986). Merely because there is a parallelism of a trait in several taxa does not invalidate the use of the trait as a shared derived trait for either taxon; if parallelism is real, the structure must by definition be a shared derived trait in each group in which it occurs. And most of the parallelisms that Brock cites involve primitive Ditrysia (Tineoid superfamilies or Cossioidea-Castnioidea-Zygaenoidea); one of the major points of Scott (1986) is that the phenetic distance between these lower Ditrysia and the Macrolepidoptera is so great that direct phylogenetic links between them are inconceivable. Brock fails to acknowledge the vast morphological gap (demonstrated by Scott 1986) between macrolepidoptera and lower Ditrysians such as Cossioidea-Castnioidea-Zygaenoidea and the Tineoid superfamilies; just counting the number of morphological differences between these groups and the Macrolepidoptera families clearly shows that they are not direct ancestors of any Macrolepidoptera, so parallelisms involving them are not directly relevant to Macrolepidoptera. Brock uses independent occurrence of many traits as justification for not proposing any phylogenetic scheme at all (his 1971 tree-like drawing resembling a phylogenetic tree was not derived from any list of characters using repeatable methods). But the fact of evolution means that the ancestor of every Ditrysia group had certain character states when it branched away from the remaining Ditrysia; therefore it is our job to deduce those character states were. Avoiding making a tree merely because of the complication of parallelism in some traits is not progress; progress is constructing trees and selecting the most likely tree, and listing the exact character changes involved so that other workers can verify or change the tree; progress is studying the characters in detail and the distribution of characters within taxa and reassigning those taxa that were misplaced (some reassignments may eliminate false parallelisms).

Brock's criticism (1988) also contains some misstatements of fact: Scott (1986) did not claim that secondary setae are absent in Noctuoidea; Scott wrote (p. 35) that "Noctuoidea...generally lack secondary setae" and his Table 1 shows that they are sometimes present. The Pyraloidea-Macrolepidoptera ancestor pupa was obtect in the sense

that only abdomen segments 5–6 moved. Sphingidae pupae occur in an earthen cell, but do any have true dense-silk cocoons? Hessel's (1969) figures clearly show that only Papilionoidea-Hesperioidea and some Cossidae have an aortic enlargement ("chamber"); the aorta is not enlarged much in other groups. The anapleural cleft IS a sulcus in Hesperioidea (fused, no longer a cleft). Abdominal segment 2 sternal apodemes are especially small in Rhopalocera. Maxillary palpi are 2-segmented in *Baronia* (Papilionidae) as well, but still they are very small in all Macrolepidoptera. Mandible remnants are not protuberant in Rhopalocera; in this group the name mandible remnant (and the erroneous name pilifer) does not represent an actual functional structure as it does in Cossoidea where the mandible remnants are definite bumps. Thus the name mandible remnant in Rhopalocera is not useful for morphological comparison, but is useful only for the convenience of Lepidopterist's descriptions; stating that mandible remnants are larger in Rhopalocera is misleading because the correct functional statement is that the sulci surrounding the absent "mandible remnant" are farther from each other. Lepidopterists' common practice of naming an ordinary expanse of exoskeleton as though it is some real functioning structure is frequently misleading; the truth is often that the area is just another undistinguished portion of body wall, and the functional structures that changed in the ancestor of the taxon are actually the sulci (which strengthen the cuticle during locomotion) or membranes (which allow movement of legs wings neck etc.)

Independently-possessed character states make the search for the true Ditrysia tree difficult, but do not justify the abandonment of the effort. Brock's comments do not mean that Scott's (1986) phylogenetic tree is wrong and should be changed. Brock should apply his expertise, and give us his phylogenetic tree, complete with character changes clearly placed on the branches of the tree (not some pseudo-phylogenetic gradistic tree conjured up with unknown methods, divorced from real data). And because parallelisms are common, perhaps a numerical taxonomy phenetic classification of the Ditrysia would be useful, merely to convince Lepidopterists that some superfamilies cannot be direct ancestors of some other superfamilies. I agree with Brock that too many Lepidoptera taxonomists refuse to apply their knowledge to study of phylogeny; these Lepidoptera taxonomists only seem to care about species/genera-level taxonomy, and once in a while they stray a bit by proposing a new obscure family; they "worship the god of genitalia" as they prepare drawings of the male and female genitalia that distinguish their species.

Literature Cited

- BROCK, J. P. 1988. Reply to Scott's criticism. *J. Res. Lepid.* 26:240-247.
SCOTT, J. A. 1986. On the monophyly of the Macrolepidoptera, including a re-assessment of their relationship to Cossoidea and Castnioidea, and a re-assignment of Mimallonidae to Pyraloidea. *J. Res. Lepid.* 25:30-38.